PROCEEDINGS
OF THE
NATIONAL ACADEMY OF SCIENCES
Volume 4 JUNE 15, 1918 Number 6

EFFECTS OF A PROLONGED REDUCTION IN DIET ON 25 MEN

I. INFLUENCE ON BASAL METABOLISM AND NITROGEN EXCRETION

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Read before the Academy, April 22, 1918

A year ago, realizing that this nation faced a food shortage, several members of the staff of the Nutrition Laboratory decided that positive evidence regarding the effect of a prolonged restriction in diet would give knowledge of possible use in an imminent emergency. Such data seemed especially important as exact experimentation on a large number of men and women, including many with peculiar dietetic habits and a supposedly low metabolism, had failed to indicate that the basal or maintenance metabolism of any particular class of persons or, indeed, of any single individual (making due allowance for differences in weight), is materially lower in energy requirement than the basal metabolism of the average individual. Since the law of the conservation of energy obtains in the human organism, it is clear that with uniform maintenance metabolism, the food requirement must also be fairly uniform. On the other hand, no evidence is available as to the actual effect of a reduction in diet, continued over a considerable period of time. Accordingly, a research was carried out by the Nutrition Laboratory during the past winter in which the effect of a low diet upon a group of normal adults was studied for a period of several months.

If the food intake is reduced below the maintenance level and the basal requirement remains constant, it is plain that there must be drafts upon previously-stored body reserves. In any study of the effect of a reduced diet, since we are dealing primarily with the question of energy rather than with a specific ingredient of the food, there must be the strictest control of the diet, so that the exact intake of energy over relatively long periods may be known. This involves, with human subjects, a degree of personal integrity and veracity that cannot be assumed but must be demonstrated.

For subjects we selected 12 young men from a considerable number of volunteers from the student body of the International Y. M. C. A. College in Springfield, Mass. The average age of the men was twenty-three years. For a period of four months these men were kept upon a much restricted diet, with an energy content of approximately one-half to two-thirds of their caloric requirements prior to the test. During the first few weeks there was a distinct drop in body weight. When the body weight had fallen on the average 12%, the calories in the diet were increased so as to prevent further loss in weight. The measurement of the caloric consumption at this sustained weight level would indicate the true caloric requirement for maintenance. For control 12 men from the same student body, living under exactly the same conditions save for the dietetic restrictions, were likewise studied and their food intake occasionally measured for periods of four or five days.

It was impossible to place all the men in respiration chambers and study their metabolism during the entire twenty-four hours, for it was realized that these men were, first, college students, and second, volunteers for scientific experimentation, and that their college duties, both intellectual and physical, must be carried out; hence they were all cautioned at the beginning of the study not to restrict their activities in any way. Even with the best conditions it could not be assumed that the muscular activity of both groups of men would be exactly alike throughout the period of observation. It thus seemed best to make the measurements of the metabolism upon a uniform basis, excluding uncertain and, more particularly, uneven muscular activity. For this purpose all of the first group of men were studied during periods of complete muscular repose and without food in the stomach, so as to obtain the basal metabolism practically every morning from the 27th of September, 1917, to the 3rd of February, 1918. The individual measurements were controlled by a group study with a large respiration chamber in the Nutrition Laboratory in which the 12 men slept every alternate Saturday night; the metabolism during deep sleep was thus obtained. No individual tests were made with the control squad, but group measurements were obtained on the alternate Saturday nights and used for comparison with the group results obtained for the diet squad.

Prior to the dietetic restriction, the basal metabolism, measured inside the large respiration chamber at night, was the same with the first and second squads, thus giving admirable proof that a group of 12 men was sufficiently large for our purpose. In addition to the observations on weight and basal metabolism, records were obtained of the total nitrogen in the food, feces, and urine during the entire four months, frequent observations were made of the pulse and respiration rate, total ventilation of the lungs, alveolar carbon dioxide, energy requirement for walking a definite distance at a definite speed, the blood (including counts of the red and white corpuscles and differentials, and determination of the haemoglobin), the blood pressure and rectal and skin temperatures. Clinical examinations were also made, as

well as measurements of the body surface, strength tests, and extensive psychophysiological examinations of the neuro-muscular processes. The effects of the prolonged reduction in diet on these various functions were striking. Those dealing primarily with basal metabolism may be summed up as follows:

1. A gradual reduction in weight to a point 12% below the initial weight took place during a period of from three to ten weeks, with low calories and a moderate amount of protein in the food intake. The normal demand of the men prior to the dietetic alteration ranged from 3200 to 3600 net calories. One squad of 12 men subsisted for three weeks on 1400 net calories without special disturbance.

2. After the loss in weight of 12% had been reached, the net calories required to maintain this weight averaged about 2300, or approximately one-third less than the original amount required.

3. At the end of the reduction in weight, the actual heat output during the hours of sleep, as computed by indirect calorimetry, was approximately one-fourth less than normal, thus giving a rough confirmation of the lowered number of calories found by actual measurement of the food intake. That there was no seasonal variation in metabolism was shown by the constancy in the metabolic level of the control squad.

4. The heat output by indirect calorimetry per kilogram of body weight and per square meter of body surface was essentially 18% lower than at the beginning of the study.

5. The analyses of food, feces, and urine were sufficiently complete to permit a nitrogen balance to be made and it was found that throughout the period of loss in weight and for some time subsequent thereto, there was a marked loss of nitrogen to the body. In round numbers these men each lost approximately 150 grams of nitrogen. There is an intimate relationship between this 'surplus nitrogen' and the metabolic level. Removing what we may designate as 'surplus nitrogen,' we believe distinctly lowers the stimulus to cellular activity.

6. The nitrogen output per day at the maintenance diet of 2300 net calories was about 9 grams. A control group of 12 men, living substantially the same life and eating in the same dining room, but with unrestricted diet, showed a nitrogen output of 16 to 17 grams per day.

7. The pulse rate was astonishingly lowered. Many of the men showed morning pulse rates as low as 33 and daily counts of 32, 31, and 30 were obtained; at least one subject gave six definite counts on one morning of 29.

8. The blood pressure, both systolic and diastolic, was distinctly lowered.

9. The skin temperature, as measured on the surface of the hands and forehead, was, with some subjects, considerably lower than normal. With most of the men normal temperatures prevailed.

10. The rectal temperature was practically normal.

The general picture that the men presented at the end of the test or at their minimum weight was one of noticeable emaciation, particularly in the face,

but all the men continued the usual college activities with no obvious reduction in stamina.

Two of the men had chronic bad noses. One was operated upon during the test and the other should have been. Aside from these two, the prevalence of colds during the period was about the same as with the other college students. During the study three men underwent ether narcosis for operations (on nose, foot, and hemorrhoids) and made rapid recoveries. One man at the lowest period of weight contracted what was diagnosed by three physicians as typhoid fever, although the final course of the disease seemed to leave the diagnosis somewhat in doubt. He ran a very high fever, and was critically ill for some time, but has made a complete convalescence and recovery and has returned to college.

The most noticeable discomfort experienced by the subjects was a feeling of cold, which it is only fair to say might be due in large part to the severity of the past winter. In general, notwithstanding the very great reduction in the metabolism (which we believe was due to the removal from the body of the stimulus to cellular activity of approximately 150 grams of 'surplus nitrogen'), the whole period of lowered food intake had no untoward effect upon the physical or mental activities of these men, and they were able to continue successfully their college duties.

The control squad, having demonstrated the absence of a seasonal variation in metabolism for about three months, were put for a period of three weeks upon a restricted ration of less than one-half their previous calorie consumption. In all details the picture exhibited by the first squad was strikingly duplicated by the second squad, although, as the loss in weight was obviously not so great (6% as compared with 12%) the phenomena were quantitatively somewhat less emphasized.

The entire research will be published in conjunction with our co-workers, Drs. Walter R. Miles, and H. Monmouth Smith, in a monograph of the Carnegie Institution of Washington in the near future.

EFFECTS OF A PROLONGED REDUCTION IN DIET ON 25 MEN

II. BEARING ON NEURO-MUSCULAR PROCESSES AND MENTAL CONDITION

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Communicated by F. G. Benedict. Read before the Academy, April 22, 1918

It is obvious that any adequate investigation of prolonged reduction in diet must include observations of the neuro-muscular processes and general mental condition of the individuals studied. In the low-diet research the psychological measurements were made at the Nutrition Laboratory on Sat-

urday evenings and Sunday mornings when the men were in Boston for the bi-weekly group metabolism experiment in the large respiration chamber.

After a standard evening meal the subjects came to the laboratory and were available during a period of approximately four hours. Of this time about one hour was spent on measurements which could be given to the men as a group. This was done in the library where table space and other conditions were adequate. Particular attention was given to the matter of lighting. The men were assigned seats which they occupied at each session although it was not believed that one location in the room was more favorable than another for the group experiments. A considerable period was allowed for preliminary adjustments, announcements, questions and a general quieting of the men before beginning the evening session.

The first measurement was a test of accuracy or steadiness of movement in tracing between parallel lines with many right-angle turns. The tempo was set by the metronome beating half seconds. The subject was instructed to start at a signal, to avoid contact with the boundary lines, and to make one straight line for each beat of the metronome. Each contact with a boundary line was counted an error.

Other measurements which could be suitably given by the group method were: Discrimination for the pitch of tones, using tuning forks and resonators as source (150 judgments); discrimination and cancellation of specified number groups on a printed page of numbers, the task being to mark each pair of successive digits which, when added, equaled 11; the addition of one-place numbers arranged in columns of ten during a test period of ten minutes; and the memory span for lists of 4-letter English words pronounced in tempo and without stress.

Psychological measurements of men on reduced diet

Group Method:

1. Accuracy in tracing between irregular parallel lines.
2. Discrimination for the pitch of tones.
3. Discrimination for specified number groups on a printed page.
4. Addition of one-place numbers for a period of ten minutes.
5. Memory span for 4-letter English words.

Individual Method:

6. Strength of grip; hands tested alternately.
7. Changes in pulse rate occasioned by short periods of exertion.
8. Latency, amplitude and refractory period of the patellar reflex.
9. Reaction time for turning the eye to a new point of regard.
10. Reaction time for speaking 4-letter words.
11. Continuous discrimination and reaction in finding serial numbers.
12. Sensory threshold for vision.
13. Sensory threshold for electric shock.
14. Speed and accuracy of the eye movements.
15. Number of finger movements performed in ten seconds.
16. Efficiency in traversing a right-angle maze.
17. Speed and accuracy in performing certain clerical tasks.

The measurements taken by the 'individual method' employed much technical apparatus. The general nature of these observations is indicated above. The apparatus was distributed in three rooms, which opened off a common hallway, and with the aid of two assistants it was possible to make psychological measurements with four of our subjects at the same time, each man serving a total of about seventy minutes. The measurements were grouped according to expediency. Nos. 6, 11, 16 and 17 (strength of grip, finding serial number, traversing the maze, and the clerical tasks) were such that two subjects could work in one room without disturbing each other. Nos. 8, 10, and 15 (patellar reflex, word reactions, and finger movements) formed a convenient group for a second room, since they all relied on the Blix-Sandstrom kymograph for chronographic record. The thresholds for vision and electric shock were determined in the third room, the main psychological laboratory. In this room, also, the distinctive measurements for the morning sessions were made. These included Nos. 7, 9, and 14, i.e., changes in pulse rate with exertion, reaction time of the eye, and speed of eye movements, together with more determinations of strength of grip and finger movement speed.

Most of the techniques used in the low-diet research, particularly those given by the individual method, had been previously elaborated in connection with other problems with the object of securing measurements and procedures which could be repeated on the same individual without large practice changes. Great care has been taken to make the measurements as objective and free from personal bias as possible; it will be noted that most of them have a distinct physiological trend. The avoidance of all practice effect and influence from changes in interest and attention is an ideal which of course is never quite reached in psychological investigation; however, the fortnight interval between sessions was a favorable circumstance for minimizing these factors in the present work.

Careful instructions preceded each measurement, both group and individual, and it is a pleasure to record the remarkable coöperation and serious attitude of the men who served as subjects.

It has not been possible thus early to get all the data into final form for comparison. Pending this it is unwise to make dogmatic statements. Final results are available for some of the measurements and the samples given in the table are thought to be representative. Separate averages for the two groups of men are presented in parallel columns headed 'D. S.' (Diet Squad; these men were on low diet from October 4, 1917, until February 3, 1918) and 'C. S.' (Control Squad; these students continued their normal diet up to January 8, 1918, and were then on reduction until January 28, 1918). The values in black face type are under normal condition of diet.

Illustrative results from psychological measurements during prolonged reduction in diet

MEASUREMENTS	NO. 1 TRACING		NO. 4 ADDITION				NO. 6 GRIP		NO. 14 EYE MOVEMENTS		NO. 15 FINGER MOVEMENTS	
	Average errors per line		Total columns in 10 minutes		Per cent of error		Average kilos for both hands		Average time in sigma		Number in 10 seconds	
Experiment lasted 4 months September 29, 1917, to February 3, 1918	D. S.	C. S.	D. S.	C. S.	D. S.	C. S.	D. S.	C. S.	D. S.	C. S.	D. S.	C. S.
	5.7	5.6	47.3	47.5	21.5	19.7					68.7	66.0
	5.9	5.6	50.2	46.9	16.3	10.9	47.6	52.8	95	92	65.6	65.2
	6.0	4.6	51.4	48.7	19.0	7.8	48.1	54.3	97	96	66.6	65.6
	5.4	4.7	54.0	49.6	19.9	10.8	46.9	52.8	97	91	64.2	64.7
	5.3	4.8	52.6	42.5	15.1	9.6	45.7	53.5	98	92	64.3	66.5
	4.1	4.4	52.7	46.9	17.0	14.0	44.9	54.1	100	95	63.4	64.2
	4.7	3.5	52.8	48.7	18.4	10.7	46.2	53.2			64.9	62.4
	4.4	3.9	52.8	51.1	14.2	8.7	45.9	53.3			65.4	61.4
	3.3		53.4		13.2		45.8				63.5	
	3.3		56.8		11.7						67.6	

Accuracy of movement in tracing between parallel lines shows a gradual improvement as the experiment progresses. The diet squad did slightly poorer in the first two sessions after the beginning of reduction and their errors are a little larger than found with the control squad up to December 8. The performance of the control squad, however, continues to improve when on the low diet, hence no definite effect is established.

As would be expected, the efficiency in adding one-place numbers gradually improved. The change, however, is remarkably small. When plotted, the curve for total number of columns added in ten minutes is almost a straight line. The most prominent fluctuation is with the control squad on their last normal date, when it so happened that the best adder (a man who had formerly been a bank clerk) was absent from the group work. The percentage of error is larger for the diet squad and contrasts with the other group in that it fails to decline proportionately as much. On the first reduction day (January 13, the men had been on 1400 net calories per day since January 8) for the control squad there was an increase in error.

In strength of grip the diet squad are definitely below the control, but the three reduction dates for the latter squad show no definite change in strength. Therefore the different level between the two squads cannot be interpreted as due entirely to the low diet condition. Unfortunately it was impossible to begin all the tests on the same date, since preliminary instruction required considerable time at the first session, and no normal is here available for the diet squad.

The eye movements, a type of muscular coördination which is peculiarly independent of voluntary control, show for the diet squad a progressive reduction in speed amounting in all to about 5%. The normal performance of the

control squad was not regular, which detracts from the significance of the lengthening in the compared two low-diet sessions.

The diet squad made their best score in finger movements on their one normal date, and from this there was a drop of about 6%, which continued irregularly until January 26. The performance of the control squad is fairly even and at a lower level than the normal for the other men and it shows also a decline on the three low-diet dates.

The many extra-laboratory observations of the men, their subjective impressions, and general physical and mental performance are of interest. Statements from the diet squad men were commonly such as these: "I feel fine, except that I notice the cold, am weak in the legs and seem to lack 'pep;'" "I find it best to study immediately after my meals, and when I begin to feel hunger, I occupy myself with the typewriter or other light physical work;" "The reduction in diet would not be hard to undergo if everyone else at the college were doing it." Opposite the impression of weakness in the legs may be set the record of one of the men, who after being on reduced diet for fifty-six days, led a hare and hound race over a course of approximately 8 miles, running the distance in fifty-four minutes, winning the race by eleven minutes. This man stated that he never felt better after a long distance run.

The men are said to have satisfactorily kept up their college work during the period. And while it might be criticised that the work would be judged leniently by those who were watching the experiment, it should be emphasized that the experiments themselves, the bother of complete collection of urine and feces, the many early morning respiration measurements, and the frequent trips to Boston, made a heavy draft on the time and energy of the men, which their fellow students did not have to bear.

On February 1, 1918, eleven of the diet squad were pitted against 11 selected men from the College at Springfield and placed in an arm-holding contest for endurance. The arms were held extended, palms down, at the level of the shoulders. The number of men falling out were practically the same in both squads; as a matter of fact, 7 of the diet squad and 8 in the competing squad were still holding their arms out in the prescribed manner sixty minutes after the trial started. It was not anticipated that the contest would be so long, and other engagements made it necessary to stop at the end of the hour. Whatever one may think of this as a real test of endurance, it is significant that the men who had been on reduced diet for four months were not appreciably inferior.

Basing judgment on the more objective laboratory measurements, in general it must be concluded that a prolonged reduction in diet produces some decline in neuro-muscular activities, but this does not seem nearly as definite nor as large as the changes in metabolism and allied measurements. The psychological changes were not such as to materially interfere with a satisfactory discharge of the common duties of student life.

EFFECTS OF A PROLONGED REDUCTION IN DIET ON 25 MEN

III. INFLUENCE ON EFFICIENCY DURING MUSCULAR WORK

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Communicated by F. G. Benedict. Read before the Academy, April 22, 1918

Of the various forms of muscular activity which might have been chosen for a quantitative study of the efficiency of the human machine under a prolonged period of reduced diet, that of walking was selected since it is the most common and necessary exercise, the element of training is practically negligible, and the results can be expressed in common and well understood terms.

The principle employed in this study consisted of the measurement of the gaseous exchange during the period of walking, and from the oxygen consumed and its known heat value for the determined respiratory quotient the energy requirement was calculated by indirect calorimetry.

The heat requirements thus found may be considered in two ways: (1) as the total cost to the individual to walk a given distance including that fraction of heat which is necessary to maintain the body organs at their normal functions and irrespective of the differences in the amount of body weight transported, or (2) it may be considered on the basis of the cost to the individual to move a unit mass of body weight a unit distance, over and above the cost for the maintenance of the body at some selected basal condition, such as lying or standing quietly. In other words it would be the cost of the superimposed work of walking. In reporting the energy requirement by the latter method it is necessary to know this resting or basal metabolism as well as that of walking.

The problem then was to determine the gaseous exchange of the subjects, at rest and walking, both before and after reduction of diet, and from the differences in the heat developed compute the average energy expended for each man under the different conditions.

Of the various methods of determining the gaseous exchange, the closed chamber principle with gas analyses for carbon dioxide and oxygen at the beginning and end of the experiment was selected for the walking experiments.

Accordingly an air-tight sheet-iron chamber was built large enough to enclose a power-driven treadmill with a man walking on it. The chamber was irregular in shape with a total volume of approximately 2400 liters. Suitable arrangements were installed for controlling the speed of the mill and recording the distance traveled and the number of steps taken.

The temperature of the chamber was determined by six resistance thermometers suitably placed and connected in series with a Wheatstone bridge and galvanometer placed in an adjoining room. Two electric fans stirred the air within the chamber while a blower capable of moving 1000 liters of air a

minute carried the air through a pipe extending from the front of the chamber to a drier and thence back into the rear of the chamber. In this pipe of moving air was placed the psychrometer and from it the gas samples were withdrawn for analysis.

During the period of walking pulse records were continuously made by means of electrocardiograms.

The carbon dioxide was determined in duplicate by means of two Haldane portable gas analysis apparatus, while the oxygen was found by means of the Sondén apparatus.

Two groups of 11 and 12 men respectively were under investigation. One squad, designated as the diet squad, was on a reduced ration from October 4 to February 3, while the other squad, known as the control squad, acted as a control until January 8 when it also went on a reduced diet until January 28.

The first experiment was made on January 6 with the 12 men of the control squad two days before their reduction of diet began. Each man walked in the chamber for twenty-four minutes at a rate close to 69.5 meters per minute. After a preliminary period of four minutes, during which time the necessary

TABLE 1

REDUCED DIET AND HEAT REQUIRED PER MAN DURING HORIZONTAL WALKING, RATE 69. METERS PER MINUTE (2.5 MILES PER HOUR)

SQUAD	CONDITION	TOTAL HEAT REQUIRED PER MAN PER KILOMETER	HEAT REDUCTION FROM NORMAL
		cal.	*per cent*
Control	Normal	62.2	
Control	20-day diet	53.5	14
Diet	4-month diet	48.4	22

adjustments were made, the experiment proper began and continued for twenty minutes. The gas samples were drawn at the start and end of this twenty-minute period and from the increase in the percentage of CO_2 and the decrease in the percentage of O_2 the respiratory quotient was found and the energy expended was computed.

The average total heat, thus measured, figured on a kilometer basis, is given in table 1 and amounts to 62.2 calories.

After this squad had been on a reduced diet for twenty days the experiment was repeated and it was then found to cost each man on an average 53.5 calories per kilometer. No experiment was made with the diet squad in September as the chamber was not in readiness at the beginning of the study, but on February 3 after four months' dieting an experiment made under the same conditions as the two preceding showed an average expenditure of 48.4 calories per kilometer.

As the average normal weights of the members of the two squads before dieting began differed from each other by only 0.5 kilogram, and since the basal metabolism of the two squads as determined in a large respiration cham-

ber under normal conditions was alike, a comparison of the three sets of experiments may fairly be made. This shows a drop in the energy expended per man per kilometer of 8.7 calories for the twenty-day diet and of 13.8 calories for the four months' diet.

That this drop in the energy requirement is not wholly due to the fact that there was less body weight to be moved may be seen by considering the heat requirements from the second viewpoint, viz., on the basis of a kilogram of body weight transported one horizontal meter.

In considering the energy requirements on this basis, it is usual to deduct from the total heat measured that portion which may be ascribed to the basal requirements of the body in a position of rest. Therefore, in addition to the series of walking experiments just described there was also conducted a series of experiments while the subjects were standing quietly. These were carried out with a portable respiration apparatus in an adjoining room immediately preceding the walking experiment. The difference between the standing and walking metabolisms may then be attributed to the superimposed

TABLE 2

REDUCED DIET AND HEAT REQUIRED PER KILOGRAM OF BODY WEIGHT IN WALKING 1 HORIZONTAL METER

SQUAD	CONDITION	HEAT PER HORIZONTAL KILOGRAMMETER	GAIN OVER NORMAL
		g.—cal.	*per cent*
Control	Normal	0.610	
Control	20-day diet	0.561	8
Diet	4-month diet	0.522	14

requirement due to walking, and from the distance walked in unit time and the body weight, the requirement per horizontal kilogrammeter may be obtained.

The three sets of experiments figured upon this basis are given in table 2. Here is shown a decreasing energy requirement from 0.610 gram calorie per horizontal kilogrammeter for the control squad in normal condition to 0.522 gram calorie for the diet squad after subsisting four months on a diet of about two-thirds normal. This reduction in the energy from 0.610 to 0.522 gram calorie per horizontal kilogrammeter amounts to an increase in efficiency of approximately 14%.

The results of these experiments are quite positive and show a marked saving in the energy requirements for walking in favor of the reduced diet whether considered on the basis of the gross energy expended which represents the real cost to the individual and to the national food reservoirs, or on the basis of the energy required per horizontal kilogrammeter. Although the results here submitted are confined to one form of muscular activity nevertheless it is believed that the quantitative results obtained would be duplicated if other forms of muscular work were studied.

POSSIBLE ACTION OF THE SEX-DETERMINING MECHANISM

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Communicated by H. H. Donaldson. Read before the Academy, November 20, 1917

Sex is the dual expression in organisms of a common series of characters which are unequally and reciprocally developed in the individuals of the two classes, the essential and primary difference between which is the presence of germ cells of two types—ova in the female, sperm in the male. Sex is not a necessity for reproduction or for biparental inheritance: it *is* required in reproduction of highly complex organisms. It exhibits many modifications of appearance and intensity in different groups and ranges from apparent correspondence of the two classes to wide extremes of dimorphism. While this gradation prevails for organisms in general, the individual is usually fixed in its status of differentiation. In exceptional cases the individual may possess the primary characters of both sexes or may alternately exhibit one or the other. The measure of difference in any case is cellular. The first observable cellular difference consists in a differentiation of conjugating cells into larger passive food-laden ova, and smaller, active sperms. Similarly the individual, ripe germ cell of either class appears definitely fixed in its character. The primary measure of difference between the two types of germ cells is now demonstrated to be nuclear, and, specifically, chromosomal. Sex is thus shown to be a cellular problem.

Any conception of the sex determining mechanism must conform to the different conditions of range and intensity of sexuality manifested by organisms. As the conditions in one type of sexuality can not be substituted for another, so the character of the determinant in one case can not be implied directly in another. The details of the mechanism are variable in correspondence with the conditions it determines.

In one type, in which the relation was first determined, the alternative character is definitely fixed and the body cells are individually and independently male or female in type. The mechanism of determination here shows itself to be characterized by a difference of one chromosome more in the female than in the male. This is a particular chromosome, marked by such peculiarities in the male as to make it readily determinable. Since this element is the measure of the differences between the male and female types of cell, and therefore the measure of the difference between the male and female animal, a study of its differential behavior should be of value in arriving at conclusions regarding the nature of sex and the method of its determination.

The two principal observations upon which an explanation of the operation of the sex determining mechanism must rest are (1) the duplex and alternative chromosome series, paralleling the double control and alternative inheritance of characters, and (2) the physical state of the active chromosomes. Sex,

being a case of strict alternative heredity, would necessarily require a method of control which should unfailingly operate to produce one or the other condition, and, since the numbers of males and females are approximately equal, it should also conform to this numerical requirement. The presence and behavior of the accessory chromosome supplies the theoretical demands of such a control. It is exactly alternative in its apportionment to the male cells, producing two equal and differentiated classes; and, aside from their unequal speed of approach to the egg, and selective attraction by it, should go into exactly half the ova. This behavior has been determined accurately and is no longer questioned.

The alternative mechanism is evident in the behavior of the accessory chromosome. It has not been clear why there should be a double condition of the sex chromosome in the female—in this respect conforming to the ordinary conditions—and a single representation in the male. The condition obtains however and in this fortunate circumstance we find the most promising approach to an understanding of the essential character of sex and the method of its determination. Of great significance here is the fact that, in the mechanism of the germ cells, there exists nothing of functional value in one sex that is not contained in the other. For the differential element of the problem we have therefore to look, not for something that is in itself male or female, but for some factor which, in operation under one set of conditions, will so control a series of characters as to give it the aspect which represents maleness; under another set of conditions the alternative state of femaleness. How then does this differentiator function?

The simplest conception of its action, perhaps, would be to ascribe to it some specific power which, exerted to a certain degree, might eventuate in the aspect of maleness, while in double that effect the series of characters would be female. In essence this is the quantitative theory. It does not however conform to any of our ideas of the alternative, or allelomorphic, action of the chromosomes. What our experience with regard to chromosome action dictates so far is this: One control for a character is sufficient for its elaboration; two controls do not exhibit the sum of their action but find themselves in opposition and usually one or the other prevails (probably for the single cell one always does); additional controls are apparently without effect.

Genetic experiments also show that in cases of sex-linked inheritance a single control in the male has the same effect as a double identical control in the female, and that, if the factors in the female are opposed to each other, such a single control as is fully effective in the male is then only half as potent.

The evidence accordingly relates to conditions of cell constitution and explanations must be in terms of cell organization and known functions. In this respect a comparison of the sex chromosomes of the male and female at the critical time when their germ cells are being prepared for union is most instructive. While in the female there is apparent no unusual activities of the sex element, as compared with the other chromosomes, in the male there are

well marked differences both in time and degree of its action. If the results of the activities of the cell are due to the ordered interaction of its parts, then any change in the rate of any one of its controlling elements, or any modification of its time of action, must essentially affect the product. The sex-chromosome during the development of the male germ cells exhibits all the signs of such a differential action. In the first stages it is much more active than the other chromosomes, as is evidenced by the large surface it exposes at the time of greatest cellular interaction. Not only is this true but it is more independent, having its substance isolated in a separate region. From this behavior it is clear why it should, in the single condition here, be quite as effective in its action as the duplex element of the female, if similar activities prevail in the body cells.

At a later stage, when it is almost certain that the new relations between controls of characters are being established, the sex chromosome is withdrawn and has its surface reduced to the least possible extent. It gives every evidence of inactivity, and, indeed, at this time has no occasion for action, since it has no homologue with which to react. In cases, unlike the Orthoptera, where there is a member to pair with it, the behavior is much the same and the genetic evidence would indicate the absence of any reaction.

Undoubtedly this is indicative of the real difference between the female and male organization, and, when fully understood, will point to the meaning of sex. It is possible that we have here the explanation of the greater variability of the male, for if a part of the control system is thus withdrawn from action at the time of reorganization it would, almost inevitably, affect the entire result. Here it may be recalled that there has been much dispute regarding the general significance of sexual reproduction, some holding that it is to ensure variation, others that it is to control or prevent it. Since both of these ends must, in some measure, be attained is it not entirely possible that the sexes represent, in part, such a division of labor? In this connection it may be pointed out that the history of the sex chromosome is such as to fit it exactly for the rôle of furthering these two purposes. It passes alternately from the male to the female line, in the one being subject to the relative instability of its unpaired condition, in the other being an orderly member of a balanced series, forced to react with its mate as do the other chromosomes. There is a possibility that in the male, the sex chromosome being unmated, or opposed by an inactive element, may be more free to react with the other chromosomes and in this way change their constitution, being in turn affected by the reaction. By the nature of its transmission it must, after this experience, pass into the female line where its relation to the complex is necessarily different. The contrast in these two conditions is obvious and the interpretation strongly suggested.

Where there is a mate to the sex chromosome in the male the genetic results would indicate that it has no function, and the very fact that it may indifferently be present or absent suggests its inactivity. This element, of all the

chromosomes, is confined to the male line and it is possible that its loss of function is due to a lack of variable reaction. It experiences in effect, a most intensive form of inbreeding and shows the characteristic results of such unvarying reactions. For it, there is no opportunity to eliminate greater or less variables, as is the case with the x-element during maturation in the female. The ultimate problem is, of course, to determine why such a difference between homologous elements should exist.

THE STUDY OF THE SEDIMENTS AS AN AID TO THE EARTH HISTORIAN

By Eliot Blackwelder

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Communicated by J. M. Clarke, March 29, 1918

Objectives of the Earth Historian.—We are, of course, still immensely far from our ultimate goal, which is a complete understanding of all the past states and events of the earth, or as Professor Salisbury used to put it, "the complete geographies of all past epochs." Progress toward this unreachable goal will be most favored if the advance is made rather uniformly, all along the front. It is true that such progress is often made by pushing out salients, but the further extension of such salients is usually impossible without corresponding support from the flanks.

In the past we have gone ahead much farther along certain lines in geologic history than along others. The history of life and of faunal succession has been cultivated assiduously for generations and is, on the whole, much better understood than other phases of the subject. Although not so well known in detail, the history of diastrophism is now fairly well blocked out and the mere continuation of studies already under way is likely to afford us in the near future a serviceable understanding of the sequence of major earth movements.

The most backward points in the general advance just now are in two sectors: That of the history of climate, and that of the principles of chronology and correlation.

The importance of climate arises from the fact that it is one of the most powerful factors, if not indeed the dominant factor, controlling not only the sculpture of the land but the nature of the deposits that are made both on land and in the sea.

Secondly, the principles of correlation must be understood better than they are now, before we can bring into their proper time relations the various events and conditions of which the sediments give us record. In spite of the impressions in elementary text books of geology, I think it will be generally admitted, by those who have carefully considered the question, that we do not yet know these principles with satisfactory accuracy. If we did, we should

not have such anomalies as the actual interbedding of strata in Montana containing marine invertebrates assigned to the Cretaceous with those containing land plants identified as Eocene; or as the Triassic in Idaho resting conformably on rocks containing only Pennsylvanian fossils. Many other cases of similar perplexity will occur to those who have had much to do with age questions in stratigraphy. All things considered, it seems to me that the improvement of correlation methods and a more general acceptance of these improvements is at the present time the thing most to be desired by the earth historian, for nowadays it is one of the chief causes of friction among us.

What service can the study of the sediments and sedimentary rocks render in connection with these two problems? First it offers the best and most comprehensive means of working out the history of climate. We already understand rather fully the climatic significance of such deposits as beds of coral limestone, of tillite, and of saline formations. We have probably reached a similar comprehension of the red beds, loess, and certain other types, although we are not yet fully agreed among ourselves regarding them. Before long we may expect to know as fully the climatic significance of the coal-bearing gray sediments, and eventually even of most of the marine deposits. For even among the, latter no matter how great the importance of the work of bacteria, algae, foraminifera, and other organisms is, it becomes increasingly evident that the very activities and processes of these organisms are largely dominated by climate, either directly or indirectly, and that they are forced to make a record of climatic changes in the marine sediments to which they contribute. For example, the prevalence of siliceous in place of calcareous ooze in the Antarctic Ocean is probably due to biochemical factors that depend on climate.

The study of climatic history is not only necessary for its own sake as a division of the larger earth history, but it has an important bearing upon the attainment of the other desideratum, namely, more reliable correlations. Climatic changes are widespread in their influence. Some, like the cooling off during the last glacial period, seem to have affected the whole earth. They influence both land and sea deposits, and hence leave their impress on all sedimentary formations. In comparison with the slow progress of geologic events, the effects of climatic change are felt quickly. We seem justified in believing that altered climates do not ordinarily migrate slowly from region to region, and on this assumption it may generally be presumed that the results are essentially simultaneous over large areas. For these reasons climatic changes should serve as very delicate indicators of time relations. They are likely to be especially valuable because their record is most clear in the terrestrial sediments, where fossils—our customary reliance—are apt to be rare or absent.

From the earliest days of geology attempts have been made to correlate strata in different places by means of lithologic character. Many of these attempts have met with either failure or only partial success. In comparatively recent years, however, more refined methods have yielded much better

results. For example, Rogers and Stone in their recent study of the Lebo shale of Montana have shown that it can be identified over large areas by its andesitic particles, which record eruptions of volcanos farther west at a single epoch in Cretaceous times.

In the future it will undoubtedly be possible to make far greater use of the physical characteristics of sedimentary rocks in correlation—not so much by a direct matching of similar rocks as by an indirect process of first elaborating from the rocks the climatic and physiographic conditions and their changes in time, and then correlating these. It should in fact be as feasible to correlate by means of climatic history as to correlate by diastrophic history. Indeed it is already beginning to be the practice of the most progressive stratigraphers to make their correlations not simply on the basis of faunas nor on the basis of diastrophism, but on the compound basis of life, climate, topography, vulcanism, and diastrophism with due regard to their mutual relations and dependences and their relative values. It is my own expectation that this practice will soon become general.

What is Needed.—We may reasonably hope to understand eventually all of the sedimentary rocks at least as well as we now know any; but at present our knowledge is very uneven, being tolerably complete for some types and very slender for others. Among the sediments which are as yet but partly understood the following may be mentioned by way of illustration of our needs: limestone conglomerates and oolites, sedimentary iron ores, chert, jasper, etc., gray flags and shales with lean faunas, dolomites, phosphorites, greensands, black oily shales, lithographic limestones, and rhythmically alternating shale and limestone. Of course, there are many others.

To advance more rapidly this part of the scientific battle front, several things are needed. Among the most useful are the prolonged and intensive studies of certain modern types of deposits and the processes of their deposition, as illustrated by the investigations that have been carried on recently by T. W. Vaughan and his associates in the Florida region, as well as by the careful study of mountain stream work by G. K. Gilbert in California. Some of these problems are too large to be attacked by most individuals, but require for their successful execution the aid of our strongest scientific institutions and the cooperation of a number of investigators over a period of years.

Hardly less valuable are the close and detailed studies of ancient sedimentary rocks, such as Barrell's interpretation of the Mauch Chunk shale, the study of the western Red Beds by C. W. Tomlinson, and of the dolomites by half a dozen or more geologists in the last few years. Probably this method must be our sole reliance in the case of certain peculiar sediments which, so far as known, are not being formed on the earth at the present time. These are illustrated by the thick, rich beds of phosphorite in Idaho and perhaps by the stratified iron ores of eastern Brazil.

Our understanding can be advanced in a most helpful way also by careful experimentation, such as that on sun-cracks reported by E. M. Kindle, or the

bacteriological experiments of Drew and others on the precipitation of calcium carbonate from sea-water and the formation of oolites. On the whole there has been much too little of this sort of work in the last few decades, perhaps because sedimentary studies have been left largely to the stratigrapher who is rarely, by force of habit, a laboratory experimenter. We shall need in this work the coöperative aid of expert chemists, bacteriologists, and others not ordinarily interested in geologic matters. The whole subject of diagenesis, or simultaneous alteration of sediments, is doubtless in the province of the bio-chemists and colloid chemists, if only we could induce them to assume the task.

It might seem, at first thought, superfluous for me to recommend the thorough combing of the geological literature for material on the nature and origin of the sediments. But it is a fact, by no means sufficiently appreciated by most of us, that the amount of buried treasure of this kind is really enormous. A few years ago, while engaged in the study of phosphorite deposits, I was astonished to find that the papers published in France and England during the seventies contained a much clearer and more comprehensive interpretation of phosphatic deposits than could be found in any American text-book or reference work published since 1900. In fact, it was perfectly evident that most even of the more valuable of these foreign papers had never been seen by the authors of the compendia mentioned, nor even by Americans who had published important papers on the phosphatic rocks. I believe it is a fact that one of the greatest services that can be rendered, just now, to the advancement of our knowledge of sediments and sedimentary rocks would be the thorough investigation, digestion, and summarizing of what is already in print on the various types of sediments. It would vastly increase our effective working knowledge and might conceivably double it, for it must be clear that a fact or a theory which is lost is as useless as one that has never been found out.

Chemical analyses give much valuable information regarding sedimentary deposits. Not infrequently the general conditions of origin as well as the nature of the rock, can be inferred at once from the analysis. We have far too few of them, and it is one of the minor discouraging conditions of the study that even those we have are in many instances rendered useless by the omission of essential facts.

Furthermore, a great many analyses are published with only the most meager information regarding the source of the material. It is obviously of very little value to the sedimentationist to know that a certain analysis pertains to a Cretaceous shale from Mt. Diablo, California, because the mountain contains several distinct types of shales of Cretaceous age. In that case even the brief statement that it was a gray sandy shale with abundant fragmentary mollusks and echinoderms would greatly enhance the value of the analysis. The same benefit might be conferred by noting that the specimen came from a particular bed in a carefully described section already published. It is for this reason that the great number of analyses in Clarke's *Data of Geochemistry*,

have only a small fraction of the value to the sedimentationist that they might just as well have had if the facts had been adequately stated. We need, therefore, more analyses, more comprehensive analyses, and especially more fully annotated analyses.

Probably our broadest, even if not our deepest, fund of information for the interpretation or sedimentary formations comes from the descriptions of sections by stratigraphers and by field geologists in general. Here, as in the case of the chemical analyses, the sedimentationist meets with frequent disappointment, simply because the exact and detailed observations, which alone could make the section valuable for his purposes, have been largely or entirely omitted. We have thousands of stratigraphic sections in which successive beds are described as 'yellow sandstone,' 'gray shale,' 'fossiliferous limestone,' etc., leaving the reader to guess as best he may the significant characteristics of the strata. Of course, the author of a report usually gives descriptions that are adequate for his own purposes even if not for those of others who may go to it for light. On the other hand those geologists who make it a practice to describe colors carefully, to note textures, the presence of cross-bedding, ripple-marks, sun-cracks, nodules, forms of grains, character of cements, nature, abundance, and distribution of fossils, to say nothing of mineral content, forms of large versus small grains, type and amplitude of cross-bedding and ripple-marks and the nature of the filling in sun-cracks, are a source of joy to the student of the sediments. Unwittingly perhaps, they are rendering us a valuable service. We only regret their scarcity.

Many of us are necessarily limited in the geographic range of our first-hand study of sediments and sedimentary processes. Others who have had the good fortune to visit distant regions, and especially the less known parts of the earth, such as the tropics and the arctic countries, can often with very little trouble collect observations and material which may later, in the hands of a trained student of sedimentation, yield important information for which we might otherwise have to wait for decades. The amount of such data now on hand in American universities and museums is very small when compared with that which is available for a study of volcanic rocks or fossil faunas.

These are some of the ways in which the study of the sediments and the sedimentary rocks can be forwarded not only by the special devotees of that branch of geology but also by anyone who takes an intelligent interest in the matter.

THE GROWTH OF THE ALASKAN FUR SEAL HERD BETWEEN 1912 AND 1917

By G. H. Parker

United States Seal Investigation, 1914

Read before the Academy, April 23, 1918

The apprehension with which the condition of the Alaskan fur seal herd was viewed some half decade ago has disappeared in consequence of the steady growth of the herd. Few animals have been more closely watched and counted from year to year than the Alaskan fur seals and their remarkable habits of breeding exclusively on the Pribilof Islands and of assembling there each summer in one immense complex family render these counts of no small biological interest. It is probable that in late July and early August of each year every living Alaskan fur seal (Callorhinus alascanus) is either on one of the Pribilof Islands or in the immediately adjacent sea. Thus an annual complete rendezvous of this species takes place in an almost unique way, and this rendezvous gives opportunity for a census of the fur seal such as is possible in scarcely any other undomesticated animal.

Table I exhibits in several particulars the numerical conditions of the herd from the summer of 1912, when detailed counting was begun, to that of 1917. The numbers in this table are taken from the successive reports on the state of the herd as given out in the publications of the United States Bureau of Fisheries (Osgood, Preble, and Parker, 1915; Bowers and Allen, 1917; Smith, 1917; *Fisheries Service Bulletin*, No. 30). The counts in the years 1912 and 1913 were made under G. A. Clark; those in 1914 under a group of six investigators, B. W. Harmon, T. Kitahara, J. M. Macoun, W. H. Osgood, G. H. Parker, and E. A. Preble; and those of 1915, 1916, and 1917 under G. D. Hanna.

Table 1 opens with an enumeration of the new-born pups for the seasons under consideration. From 1912 to 1916 inclusive these enumerations were made as direct counts of the numbers of pups on the beaches by methods well established on the islands. In 1916, owing to the increase in the number of pups, direct counting was accomplished only with difficulty, and in 1917, in consequence of still greater increases, it was found necessary to resort in part to a method of estimates. Hence it is believed that the numbers for 1916 and particularly for 1917 are not so accurate as those for the preceding years.

In no feature is the growth of the herd indicated more clearly than in the yearly increase in pups. This increase ranges in a progressive series from 81,984 in 1912 to 128,024 in 1917. The nature of this increase can be appreciated best when the numbers are plotted in some such way as in Graph 1 in which the abscissas represent years and the ordinates numbers of pups in

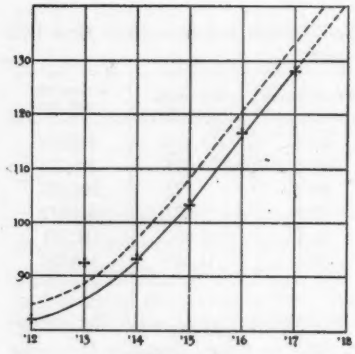
TABLE 1

NUMERICAL STATEMENT OF THE CONDITION OF THE ALASKAN FUR SEAL HERD FROM 1912 TO 1917 INCLUSIVE

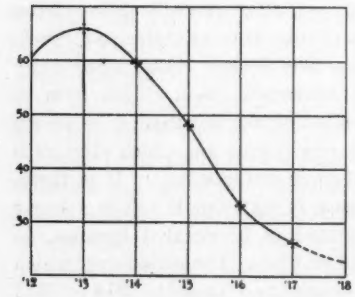
YEAR	PUPS	HAREM BULLS	AVERAGE HAREM	IDLE BULLS	ESTIMATED TOTAL HERD
1912	81,984	1,358	60.4	113	215,738
1913	92,269	1,403	65.8	105	268,305
1914	93,250	1,559	59.8	172	294,687
1915	103,527	2,151	48.1	673	363,872
1916	116,977	3,500	33.4	2,632	417,281
1917	128,024	4,850	26.4	11,683	468,692

thousands. Here it will be seen that the successive counts, with the exception of one, lie almost exactly on a curve such as would describe the commencement of an autocatalytic process. This type of curve when complete shows in the beginning an accelerated increase, after which it approximates more nearly to a uniform rate till a decline of this rate sets in due to a checking of autocatalysis by retarding factors. The decline ceases when autocatalysis is balanced by the conditions unfavorable to it. This form of curve is usually applied to the growth of an individual, but there is no reason why it should not apply also to the growth of a population, which after all is protoplasmic growth and hence dependent upon autocatalysis. It is, therefore, not surprising to find that the increase in pups should follow a curve characteristic of such a process. The period of accelerated increase, as Graph 1 shows, extended from 1912 to 1914 or 1915. The period over which a more nearly uniform rate of increase was maintained began in 1914 or 1915 and extended to the last reported count, 1917. How much longer it will continue cannot be stated. Eventually, as numbers augment, the beaches on which the breeding occurs will become overcrowded, shortage of food may supervene, epidemics due to unfavorable conditions may appear, and these and other like influences will cut down the rate of increase until the herd, having arrived at its maximum number, will stand at a constant level. Just as it is impossible to predict how long after 1917 the steady increase will be maintained, so also it is impossible to foretell when equilibrium will be reached.

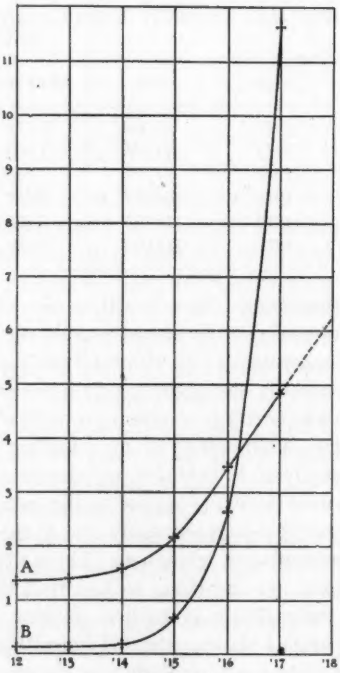
The only pup count that fails to fall in line with the interpretation of the growth of the herd just given is that of 1913. This count when considered in relation to the other counts is some six thousands too high. When the pup census of 1914 was made public, it was pointed out (Osgood, Preble, and Parker, 1915, p. 41) that the increase of this year over the preceding one was very slight, as a matter of fact only a little over 1%, whereas the increase of 1913 over 1912 had been 12.5%. Clark (1916, p. 608) also commented critically on these numbers and stated that "The results gave a gain of only 1%, without any adequate explanation for the irregularity," the implication being that the count of 1914 was deficient. Now that there are in all six counts that may be compared, it is quite obvious, as an inspection of Graph



GRAPH 1



GRAPH 3



GRAPH 2

GRAPH 1. ALASKAN FUR SEAL PUPS BORN IN THE SEASONS 1912 TO 1917 INCLUSIVE

The abscissas represent years and the ordinates numbers of pups in thousands (table 1). The actual numbers counted, except those for the year 1913, fall on a curve represented by the full line which has been extended one year by extrapolation. All counts on the curve are probably low, the dotted curve indicating the direction of their real values.

GRAPH 2. NUMBERS OF HAREM BULLS (A) AND OF IDLE BULLS (B) IN THE ALASKAN FUR SEAL HERD OF THE SEASONS 1912 TO 1917 INCLUSIVE

The abscissas represent years, the ordinates numbers of bulls in thousands (table 1). The curve for harem bulls (A) is extended one year by extrapolation.

GRAPH 3. THE AVERAGE HAREM IN THE ALASKAN FUR SEAL HERD OF THE SEASONS 1912 TO 1917 INCLUSIVE

The abscissas represent years, the ordinates numbers of females in the average harem. The curve has been extended one year by extrapolation.

1 shows, that it is not the count of 1914 that is anomalous but that of 1913. Why this should be so extraordinarily high is difficult to state. It is perfectly clear to any one who has counted pups on the rookeries of the Pribilofs that even the most accurate count is bound to fall short of the real number, so that the magnitudes indicated by the dotted line between 1912 and 1917 in Graph 1 show the directions in which the real numbers lie rather than the solid line of actual enumerations. But even admitting that all the enumerations, except that of 1913, are low, there is no reason to suppose that these are so far low as would be implied on the assumption that the count of 1913 is the most nearly accurate count of them all. It seems impossible that Clark should have improved his method so much between 1912 and 1913 as to have found as large an increase as is implied in the count of 1913. Such increases based on assumed improvement in method do not appear among the several counts by Hanna. But in whatever way this discrepancy in the counts may be explained, it must at least now be clear that the count of 1914 agrees well with the majority of the other enumerations and that the count that is exceptional is that of 1913.

Another way of indicating the growth of the herd as seen in the numbers of pups, is the percentage of annual increase in this constituent. From time to time statements have been made as to what this percentage under normal conditions should be. Thus Clark (1917, p. 499), selecting the increase in 1913 (12.5%) and in 1916 (13.0%), concluded that these figures "may be taken as fixing with reasonable exactness the rate of growth at about 13%." Hanna (Bower and Allen, 1917, p. 92) has expressed the opinion that about 12% is the normal rate of increase. How these numbers compare with the actual figures of the last five years can be seen in table 2.

TABLE 2

INCREASE OF PUPS IN THE ALASKAN FUR SEAL HERD FROM 1913 TO 1917 INCLUSIVE

YEAR	NUMBER OF PUPS IN THE		ACTUAL INCREASE	PERCENTAGE INCREASE
	GIVEN YEAR	PRECEDING YEAR		
1913	92,269	− 81,984	=10,285	12.5+
1914	93,250	− 92,269	= 981	1.1−
1915	103,527	− 93,250	=10,277	11.0+
1916	116,977	−103,527	=13,450	13.0−
1917	128,024	−116,977	=11,047	9.4+
General average.....				9.4

Here it will be noted that the percentage increase varies from 1.1 to 13.0 with an average of 9.4. This computation included the very anomalous count of 1913, but even if this were replaced by a number such as would be indicated by Graph 1 (86,000) such a replacement would not materially alter the general average. The percentage increase in 1913 would then be 4.9

instead of 12.5 and in 1914, 7.8 instead of 1.1 and the general average would be 9.2 instead of 9.4. Thus for the last five years the annual increase of pups has averaged a little over 9% and is probably nearer 9.5 than 9. Hence both Clark's figures and Hanna's are obviously too high for the present state of the herd. In fact Clark's assumption of 13% is quite unwarranted for he gives no reason for accepting this number rather than the other chosen by him, 12.5.

The average rate of increase indicated in table 2, 9.4%, would bring about a doubling of the number of pups in approximately eight years. If no untoward conditions arise, it is fair to expect that in 1920 about twice as many pups will be born as were born in 1912.

Beside the annual increase in the number of pups born, the Alaskan fur seal herd has given evidence of growth in the increase of its harem bulls. These bulls, which are the sexually mature males that have succeeded in having associated with them one or more breeding females, are perhaps the most accurately counted element in the whole herd. The determination of their numbers between 1912 and 1917 was made by direct count. The results of this census are given in the column in table 1 marked 'harem bulls' where it will be seen that they represent an increasing series running from 1358 in 1912 to 4850 in 1917. The distribution of their rates of increase describes a curve (Graph 2, A) not unlike that seen for the pups. An extrapolation on the basis of this curve leads to the prediction that the number of harem bulls in 1918 will be somewhat over 6000.

The average harem for a given season is the average number of females associated with the harem bull of that season. It is a derived number found by dividing the number of females as indicated by their pups for a given season by the number of harem bulls for that season (table 1). If the males of the herd have been considerably reduced in numbers, as might result from excessive killing, the number in proportion to that of the females would be naturally small and consequently the average harem would be large.

Such a condition, if excessive, would be an unfavorable sign in the herd and improvement would be marked by a decrease in the size of the average harem. The course of events in this particular between 1912 and 1917 is represented in table 1 the details of which can be better appreciated by reference to Graph 3. In this it will be seen that the average harem presented its most unfavorable condition in 1913 after which there was a steady improvement to 1917 in that the average number of females to each harem bull fell from 65.8 to 26.4. It is to be remembered, however, that the extremely unfavorable point in the curve, 1913, is dependent upon the anomalous pup count of that year. Had this count been in line with the others, the average harem for 1913 would have been very near those of 1912 and 1914. The subsequent change, however, would not have been affected and this reduction can not be looked upon as anything but a favorable sign. From the extrapolation in Graph 3, there is good reason to believe that the average harem will be even smaller in 1918 than it was in 1917.

The idle bulls, to turn to another element in the herd, are those males that have attained breeding age but that have failed to obtain one or more females. They usually occupy less favorable areas on the outskirts of the rookeries and may even move from place to place. They can be counted with almost as much accuracy as the harem bulls, but their occasional migrations make their count somewhat uncertain. In 1917 the idle bulls were so numerous that it was deemed wise to subdivide them into two classes, idle bulls proper or those with fixed positions on the breeding grounds but without females, and surplus bulls or those that were unable to find positions on the breeding ground and that usually resorted to other parts of the beaches notably the bachelors' hauling grounds. These two classes are combined in table 1, 1917, under the single head 'idle bulls.'

The idle bull indicates a maladjustment in the breeding conditions of the fur seal. This feature has already been pointed out as evidence of imperfect adaptation in this species (Parker, 1915). As already stated, in the fur seal, as in most other higher animals, the numbers of males and females at birth are very nearly equal. When the breeding period arrives, however, one male associates himself with a large number of females, the lowest average harem in the last five years being over 26 females to one male. Consequently, notwithstanding the fact that the male breeding period is only six to eight years (Clark, 1916, p. 608) as contrasted with the longer female period of approximately ten or eleven years, many males do not procure females. Thus the idle bulls are a measure of this natural maladjustment within the herd. In a wholly natural state of the herd they would undoubtedly be represented by considerable numbers. Their presence, at least in large numbers, can never be anything but a detriment. They are continually stirring up strife not only among themselves but also among the breeding bulls and they are accountable for the maiming and the death of many young seals. They are the individuals that in the period of their best pilage should have been killed for their skins and their excessive numbers indicate poor management of the herd. Their history during the period under consideration is shown in table 1 and more strikingly in Graph 2, B. Here it will be seen that the numbers of idle bulls remained small from 1912 to 1915 after which it increased considerably in 1916 and enormously in 1917.

This constitutes the one unfavorable feature in the recuperation of the herd, for it marks the effective appearance on the beaches of the first real element that is detrimental. Fortunately it is within reasonably easy control, for the fur seal herd is open to the same kind of management that chickens or cattle are. In these stocks, as in the fur seals, the sexes are approximately equal at birth and in both instances, although good management calls for a careful rearing and preservation of females, it also demands the retention of only such males as are necessary for breeding, the excess being drawn off for market purposes. This is clearly what should be done with the surplus male seals, a step that out government is now prepared to take (Smith, 1917, p. 92).

It is only to be regretted that this step was not taken earlier, for the large number of idle bulls now in the herd is both a detriment to that body and a positive loss of once good skins. This menace in the growth of the herd has been repeatedly pointed out (Clark, 1914, 1916, 1917; Osgood, Preble, and Parker, 1915) to those having the matter in charge.

The last column in table 1 contains the estimated annual totals for the herd. These numbers are highly artificial in that they are largely made up of computed elements and they are, therefore, so remote from direct observation that their detailed consideration is scarcely worth while. The fact that all elements of the herd have separately increased between 1912 and 1917 is reflected in the increase of these calculated totals from 215,738 in 1912 to 468,692 in 1917.

In conclusion it may be stated that since 1912 the steady increase in the numbers of pups born, and of harem bulls and the decrease since 1913 of the average harem are most favorable signs in the growth of the herd. The one unfavorable feature during this period is the considerable increase in idle bulls in 1915, 1916, and especially in 1917. This increase, which can be eventually checked, shows that active commercial killing should have been restored some years ago.

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THE DESTRUCTION OF TETANUS ANTITOXIN BY CHEMICAL AGENTS

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Communicated by R. Pearl, April 13, 1918

The ultimate object of this work is a solution of the problem of the chemical nature of antitoxins and their preparation in the pure state. That this would be attained was not expected in view of the numerous previous investigations which left these problems unsolved. But it seemed highly probable that data would be obtained which would throw some light on the subject.

Up to the present time numerous investigators attempted to separate antitoxins from their associated proteins, but without complete success. The

well known tetanus and diphtheria antitoxins are examples of preparations containing all or nearly all of the immunity units present in the original serums, but only a part of the proteins. Thus, Homer[1] (p. 400) concentrated a tetanus serum containing 100 units per cubic centimeter and 6% of protein, obtaining an antitoxin that contained 900 units per cubic centimeter and 19% of protein. In this process 10% of the antitoxic units were lost, the antitoxin was nine times as potent as the serum from which it was derived, but it contained only three times as much protein. The failure of all attempts to obtain a protein-free antitoxin preparation has led some investigators to the conclusion that the antibody or group of antibodies which constitutes the antitoxin is one of the serum proteins and hence cannot be completely separated from protein. The concentration of the antitoxin without a similar concentration of protein is regarded by others as an indication that the antitoxin may be a body of non-protein nature.

Under these conditions any test which would conclusively decide whether an antitoxin is or is not identical with a serum protein, would have both a practical and a theoretical interest. The following test was decided upon because of its promising nature. If an antitoxin, tetanus antitoxin, for example, is a substance of non-protein nature, it should be possible to prepare artificial digestion mixtures containing the antitoxic serum or antitoxin derived therefrom, in such fashion that the protein would undergo digestion without loss of antitoxin. Appropriate chemical measurements would indicate the extent of proteolysis, while inoculation experiments on guinea pigs would indicate whether there was any loss of antitoxic units. If, on the other hand, the antitoxin is a protein and its power to immunologically neutralize the corresponding toxin is a function of the intact protein molecule, then the antitoxin would be destroyed in every case where the protein had undergone cleavage, regardless of whether the cleavage was caused by a proteolytic enzyme or other chemical agent. Due regard must of course be had for the possible destruction of the toxin by the chemical agents used.

Several different antitoxic preparations were exposed to the action of trypsin-sodium carbonate solution or to pepsin-hydrochloric acid for comparatively long periods of time, with suitable controls. At proper intervals the extent to which digestion had taken place was measured chemically and compared with the loss of antitoxic units; the latter were determined by guinea pig inoculation as described by Rosenau and Anderson[2]. The results may be summarized as follows:

1. Tetanus antitoxin in 0.5% sodium carbonate solution was slowly and completely destroyed. At the same time no significant chemical changes in the proteins were detected.

2. In solutions amphoteric or faintly acid to litmus strips, trypsin destroys the antitoxin and at the same time the associated proteins are digested. The rates of antitoxin destruction and protein splitting were substantially the same.

From (1) it follows that antitoxin destruction may take place with or without protein splitting.

3. In solutions containing trypsin and 0.5% sodium carbonate the results were the same as in (2).

4. Tetanus antitoxin in 0.2% hydrochloric acid was completely destroyed in three or more days. During this time no significant chemical changes in the proteins were detected.

5. In neutral solutions pepsin did not affect the antitoxin.

6. In pepsin-hydrochloric acid, proteolysis and antitoxin destruction proceeded simultaneously.

These results tend to indicate that tetanus antitoxin is a substance of non-protein nature. But the stability of the antitoxin is so dependent upon that of the protein to which it is attached, that whenever the protein molecule is split, the antitoxin splits with it.

The experimental details are given in the *Journal of Agricultural Research*, 1918.

[1] Homer, A., *J. Hygiene, London*, 15, 1916, (388–400).
[2] Rosenau, M. J., and Anderson, J. F., *Hygienic Lab. Bull.*, No. 43, 1908.

TESTS FOR FLUORINE AND TIN IN METEORITES WITH NOTES ON MASKELYNITE AND THE EFFECT OF DRY HEAT ON METEORIC STONES

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Communicated by E. W. Morley, April 29, 1918

The following is a partial report on results obtained in continuation of work under a grant from the J. Lawrence Smith Fund of the National Academy of Sciences.

1. On Fluorine in Meteoric Stones.—So far as I am aware the occurrence of fluorine has never been recognized in meteoric stones. Meunier[1] records its presence as doubtful. Fletcher in his Handbook does not mention it at all, nor is it mentioned by Cohen nor by Lockyer in his Meteoritic Hypothesis as one of the elements even recognizable by the spectroscope. Nevertheless, the occurrence of a calcium phosphate has often suggested its possible presence, and the wide distribution of this phosphate in meteoric stones, which I have shown of late[2] seemed to warrant further tests, particularly as new and more refined methods for its detection had been discovered. Opportunity for these tests was recently afforded by Dr. E. T. Wherry when engaged upon the investigation of some fossil bones for Dr. Hrdlička in the Museum laboratories. The method consists in the digestion of the material in concen-

trated sulphuric acid in a small flask heated to 200°C. in a paraffine bath, the process being continued for four to five hours. The gases evolved are bubbled through water, the fluorine being retained in water solution (in a U tube). In ordinary work it is customary to titrate the solution thus obtained with a standard alkali, but inasmuch as the meteoric samples tested contained both chlorides and sulphides, which would yield hydrochloric acid and sulphur dioxide, it was necessary to make the solution first alkaline with sodium hydroxide and evaporate nearly to dryness in a platinum dish on the water bath, the resulting concentrated solution being then added to a standard peroxidized titanium solution in a colorimeter. Fluorine has the power of decolorizing or at least reducing the intense yellow color of this titanium solution, even when present to an amount not exceeding 0.001%.

Three samples were tested—Bluff, Texas; Allegan, Michigan; and Waconda, Kansas, in each of which phosphoric acid (P_2O_5) to the amount of 0.25% has been recorded, and in all of which the phosphate had been recognized microscopically. Amounts of from 10 to 20 grams were used in the tests, and in not a single instance did the titanium solution show the least sign of the presence of fluorine. It would seem safe to assume, then, that in these cases at least the element was not present.

2. Further Tests for Tin in Meteorites.—It will be recalled that in my report on previous investigations, I stated that no traces of tin had thus far been found by us. Incited, however, by the work of Derby[3] I was led to follow up the matter still further. Derby, it will be remembered, reported 1.18% tin in the schreibersite of the Cañon Diablo iron. Concerning this, he states, "Tin has not been reported (i.e., previously) possibly because the solution has usually been made in aqua regia in which it would only appear through a special research. In the present case, the solution was made in plain nitric acid and the tin appeared as oxide and was verified by blowpipe tests."

Having obtained a considerable quantity of material, chiefly schreibersite and cohenite with some carbon from various digestions of the Cañon Diablo iron in dilute hydrochloric acid, I submitted it to Dr. Whitfield with the request that he examine the same with no other end in view than the determining of tin, if present. The results were negative. Two lots of 5 grams each were taken and dissolved in nitric acid as described by Derby. Not a trace of tin could be found, either in the first solution, as oxide, or by treating the solution with hydrogen sulphide, in the customary way.

On the assumption that there is no error in Derby's work, we must assume as suggested by him, that the tin does not belong to the schreibersite but to another mineral that is not generally distributed throughout the meteoric mass so that it only appears in certain portions of the residue.

3. On Maskelynite.—In a very large proportion of the stony meteorites I have described, or studied, mention is made of a colorless, interstitial material either quite isotropic or slightly doubly refracting, with rather low index of refraction, which, following Tschermak and others, I have called, though

often with mental reservations, maskelynite. The material first described under this name, it will be remembered, was found as a prominent constituent of the meteorite of Shergotty, and was sufficiently abundant to allow a satisfactory determination of all of its properties, including chemical composition. All occurrences since noted are of microscopic dimensions—mere interstitial areas of rarely more than two or three millimeters in diameter, and determinable properties so nearly negative that the referring of the mineral to maskelynite has been more in the nature of an acknowledgment to the quality of Tschermak's work than to actual determinations on the part of those describing it. This certainly was true in my own case,[4] and it was not until I was studying the stone of Holbrook, Arizona (1912), that I separated particles and by the recently introduced immersion method determined the index of refraction to be 1.51, which, according to Larsen's tables, is that of an oligoclase glass. Since that writing I have followed up the matter as systematically and thoroughly as time and opportunity will permit, and have reached the conclusion, pronounced without hesitation, that the mineral is in all cases feldspathic, ranging in composition from oligoclase to anorthite, and owes its condition to a fusion since the original crystallization of the stone, followed by a cooling too rapid to allow it to regain its normal properties.

These conclusions are based upon examinations of a large number of sections in which I have found the mineral in all stages from a glass essentially isotropic with the low index (1.51) mentioned above to one plainly biaxial but without crystal outlines, cleavage, or other recognizable properties, with indices of 1.543 and 1.545, and in one case (Ness Co.) 1.56. Also in forms where the mineral is largely isotropic but still retains, in places, traces of plagioclase twinning. It is this last feature, it should be stated, that causes me to consider it a re-fused feldspar, rather than a residual and original feldspathic glass.

These observations, it will be observed, are supplemental and corroborative of those of Tschermak.[5] The subject seems worthy of this extended notice, not merely on account of the new observations, but since Farrington in his recent *Meteorites* remarks concerning the mineral that "Its exact nature is yet to be determined."

Attention should be called, in this connection, that an elevation of temperature sufficient to fuse a feldspar without at least partial destruction of the olivine would be impossible but in an atmosphere completely devoid of all oxidizing gases. (See further under Effects of Heating, below.)

4. Effects of Heating Meteoric Stones at Various Temperatures.—The fact that Meunier had transformed meteoric stones of his aumalite group into tadjerites, by heating to redness, suggested the availing myself of opportunities offered by the Pennsylvania Zinc Company at their works at Palmerston, Pennsylvania. A series of prepared specimens, including two cubes, one each of the Estacado and Homestead meteorites, some 10 mm. in di-

ameter, were introduced into pits bored in fire-brick, and sealed up with fire-clay. These bricks were then placed on top of a gas-fucl zienc smelter, where they were allowed to remain for a period of four months, and in an atmosphere in which oxidizing influences were reduced to a minimum. The exact temperature could not be determined, but a cube of the Casas Grandes iron in one of the pits was completely fused and absorbed into one of the bricks, indicating a temperature not less than 1450°C. The results on the two stones were as follows:

Estacado, Texas.—This, a veined crystalline chondrite of a dark gray color, fine and compact texture, consists essentially of olivine and enstatite with smaller amounts of pyrrhotite and nickel-iron. Before heating, the silicates are colorless and limpid, and the metallic constituents scattered in small granules fairly uniformly throughout the mass. The roasting resulted in producing a slight glaze on the exterior surface of the cube. The color was much darkened, becoming uniformly dull black. Although remaining firm and hard, the stone became filled with fine vesicles. The thin section under the microscope seemed at first completely amorphous. In strong sunlight, however, the silicates, although nearly opaque and without action in polarized light, were of a deep dull red, indicating a certain amount of oxidation of the iron. The interstices were filled with a fine, dust-like, opaque and amorphous matter which is impossible of determination. The particles of metal had been fused and the material diffused throughout the ground to appear in the form of minute blue points in reflected light. With the exception of the metal not a single one of the original constituents was recognizable.

Homestead, Iowa.—This is a dark gray, homogeneous, hard and firm stone belonging to Brezina's brecciated gray chondrite group. The mineral composition, as determined by Wadsworth, is olivine, enstatite, pyrrhotite, iron, and base. Lasaulx is quoted as having noted the occurrence of a feldspar. I find, in addition, a polysynthetically twinned pyroxene and a calcium phosphate in small quantities. After the roasting, the color is dull black and the texture finely vesicular, although firm and hard. As was the case with the Estacado stone, it is so opaque in thin sections as to almost entirely obscure its original structure. The silicates are altered in the same manner, though the olivine is the most affected; the metal is likewise diffused, and attempts at making a photo-micrograph from the slides resulted in complete failure. As it was evident the experiment had been carried too far a second attempt was made in the museum laboratory at lower temperatures and for shorter periods.

Small pieces of the Homestead meteorite, of 2 or 3 grams weight, were roasted in a covered crucible at a low red heat for periods of one-half and one hour each, the gas flame playing freely up and around the crucible. The external manifestations of this heating were a change in color to dark gray and a fusion, on the outer surface, of the troilite granules. A thin section showed the nickel-iron to be unchanged, though the troilite had been broken

up to a greater or less extent. The principal change lay in the finer siliceous material occupying the interstices of the other minerals which had turned, in part, black and amorphous. A like change had taken place along the borders of the chondrules and cleavage and fracture lines of the larger phenocryst, giving them the appearance of having been injected with some dark coloring fluid. The amount of change was proportional to the length of time in heating, as shown in figures 1, 2 and 3.

Figure 1 is from the unaltered stone and 2 and 3 from fragments heated for one half and one hour respectively. It will be noted that there is an increase in the amount of black opaque matter in 2 and 3 over that in 1. In the piece roasted for a full hour the fine interstitial silicates have become wholly changed to the black matter which penetrates the borders of the chondrules and other crystal aggregates, until a condition is reached so closely resembling that shown in sections of the McKinney (fig. 4) and Travis County (fig. 5) black chondrites as to apparently leave no doubt as to the correctness of Meunier's view to the effect that such are but phases of chondritic stones which have been altered through a re-heating subsequent to their first crystallization. It should be added that these roasted pieces are partially restored to their original color by digestion in hydrochloric acid and sodium carbonate, showing that the change is one that has influenced chiefly the olivine. Incidentally attention may well be called to the manner in which the blackening of the stone first manifests itself along cleavage and fracture lines in figure 5.

It should be noted further that it is doubtful if all of the dark color in these black chondrules is due to roasting, since some of them heated in a closed tube give evidence of the presence of a small amount of a hydrocarbon.

[1] *Encyclopédie Chimique*, **2**, appendix 2, Meteorites.

[2] *Amer. J. Sci., New Haven*, **43**, 1918, (322).

[3] *Ibid.*, **49**, 1895, (101–110).

[4] See my papers on the meteorites of Holbrook, Arizona, *Smithsonian Misc. Coll., Washington*, **60**, No. 9, 1912; Modoc, Kansas, *Amer. J. Sci., New Haven*, **21**, 1906, (356–360); Rich Mountain, North Carolina, *Proc. U. S. Nat. Mus., Washington*, **32**, 1907, (241–244); Thomson, Georgia, *Smithsonian Misc. Coll.*, **52**, 1909, (473–476); Fisher, Minnesota, *Proc. U. S. Nat. Mus.*, **48**, 1915, (503–506); and Coon Butte, Arizona, (Mallet), *Amer. J. Sci.*, **21**, 1906, (351).

[5] See Cohen's *Meteoritenkunde*, pp. 311–314, Figure 2 on plate 17 or Tschermak's *Die Mikroskopische Beschaffenheit der Meteoriten* will apply equally well to the majority of occurrences.

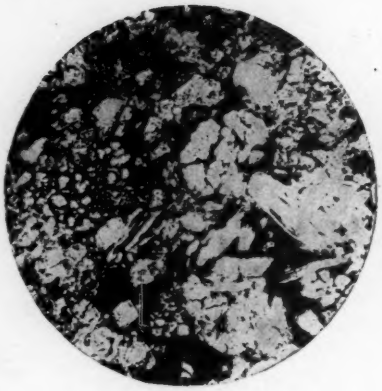

FIG. 1

FIG. 2

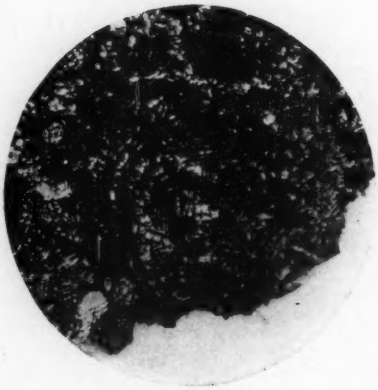

FIG. 5

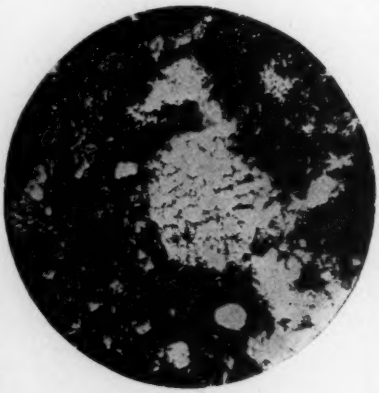

FIG. 3

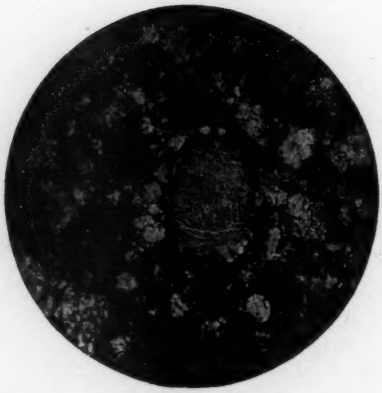

FIG. 4

NOTES ON ISOTOPIC LEAD

By Frank Wigglesworth Clarke

United States Geological Survey, Washington

Read before the Academy, April 23, 1918

One of the most remarkable discoveries in the field of radioactivity, has been the fact that the elements of highest atomic weight, uranium and thorium, are unstable, and undergo slow transformations into other substances; especially into helium and lead. The lead thus produced is identical with normal lead in its spectrum and its distinctively chemical properties, but different in its atomic weight; and this difference, which is thoroughly established, is of peculiar significance. The purest lead from uranium minerals has an atomic weight fully a unit lower than that of ordinary lead, while that from thorium minerals is nearly a unit higher. These are the extreme differences, so far as the present evidence goes; but the actual determinations of the atomic weights of these *isotopes* of lead show wide variations due to differences between the minerals from which the lead was obtained. Furthermore, these isotopes differ from ordinary lead in specific gravity; one being lighter and the other heavier than ordinary lead, these differences being proportional to the variations in atomic weight. Consequently the three kinds of lead have the same atomic volume, and occupy the same place in the periodic classification of the chemical elements.

Ordinary or *normal* lead differs from isotopic lead in one important respect, namely, its atomic weight is constant, and the actual determinations vary only within the limits of experimental uncertainty. This constancy was established by Baxter and Grover,[1] who studied lead from a number of distinct sources. Their material was derived from four mineral species; galena, cerussite, vanadinite, and wulfenite, and also from commercial lead nitrate. Furthermore, the minerals examined came from seven widely separated localities; two from Germany, and one each from Australia, Missouri, Idaho, Washington, and Arizona. The lead in each case was carefully purified, and converted into chloride, with which the determinations of atomic weight were made. The method of determination was the standard method long in use at Harvard, and based upon large experience and the most thorough technique. The values found for the atomic weight are shown in the following table:

Source	*Atomic weight of lead*
Commercial nitrate	207.22
Cerussite, New South Wales	207.22
Cerussite, Eifel Mountains, Germany	207.20
Galena, Joplin, Missouri	207.22
Cerussite, Wallace, Idaho	207.21
Galena, Nassau, Germany	207.21
Vanadinite and wulfenite, Arizona	207.21
Galena, Metalline Falls, Washington	207.21

A series of analyses of lead bromide gave values practically identical with these.

This evidence as to constancy of atomic weight is conclusive, but it has also been confirmed by three investigations by Richards and his colleagues in this country, and by Hönigschmid in Vienna. These later determinations were made as checks upon determinations of the atomic weight of isotopic lead derived from uranium minerals.

In 1914 Richards and Lembert[2] published their determinations of the atomic weight of isotopic lead. Their results may be summarized as follows:

Source	*Atomic weight*
Lead from Ceylonese thorianite	206.82
Lead from English pitchblende	206.86
Lead from Colorado carnotite	206.59
Lead from Bohemian pitchblende	206.57
Lead from North Carolina uraninite	206.40

Two years later another series of determinations by Richards and Wadsworth[3] appeared. The average results obtained were as follows:

Source	*Atomic weight*
Australian carnotite	206.375
Colorado carnotite	207.004
Bröggerite, Norway	206.122
Cleveite, Norway	206.085

Still another series of six determinations by Richards and Hall[4] on lead from Australian carnotite gave a mean value for the atomic weight of Pb = 206.415.

In a preliminary study of lead from Bohemian pitchblende, Hönigschmid[5] found values for the atomic weight ranging from 206.719 to 206.749. In a later investigation by Hönigschmid and Horovitz,[6] lead was extracted from three different minerals, namely, the purest Joachimsthal pitchblende, a crystallized uranium ore from Morogoro, German (?) East Africa, and bröggerite from Norway. The average values for the atomic weight were as follows:

Source	*Atomic weight*
Pitchblende	206.406
The Morogoro ore	206.042
Bröggerite	206.067

All of these determinations of atomic weight, including those of Baxter and Gover on normal lead, were made by the same method, the same care as to purity of materials, and the same refinements of technique. Even Hönigschmid, now in Vienna, had worked on atomic weight determinations with Richards, and so was familiar with the best procedure. The results obtained are therefore strictly comparable.

For the atomic weight of thorium lead the data as yet are scanty, and based entirely upon material derived from Ceylonese thorite and thorianite.

From the specific gravity of thorite lead Soddy[7] has deduced the atomic weight of Pb = 207.64; and Hönigschmid[8] from analyses of lead chloride prepared from Soddy's original material has found Pb = 207.77. This value, however, is probably too low for the true thorium lead, for the reason that thorite, with a preponderant proportion of thoria, also contains some uranium. The thorite lead, therefore, must contain both isotopes, but with the higher one in much the largest quantity. The thorianite lead studied by Richards and Lembert had a still lower atomic weight, namely Pb = 206.82, which shows that this variety of the metal is not of uniform character.

That the atomic weight of uranium lead is extremely variable has already been shown. In order to interpret this variability its sources must be studied both geologically and mineralogically. On the geologic side of the question the uranium ore can be divided in to three principal classes, which are sharply distinct. The definitely crystallized varieties of uraninite occur in coarse pegmatites, associated with feldspar, quartz, mica, beryl, and other minor accessories. The massive pitchblende is found in metalliferous veins, together with sulphide ores of copper, lead, iron, zinc, and so forth. As for carnotite, that is a secondary mineral, found commonly as an incrustation on sandstone, and often, also upon fossil wood. There may be other modes of occurrence, but these are the most distinctive.

In chemical composition the uraninites, as shown by Hillebrand's[9] splendid series of twenty-one analyses, fall into well defined groups. All contain uranium oxides, ranging from 65 to 90%, the low figures, however, representing altered material. The crystallized, pegmatitic uraninites are characterized by their content in thoria and other rare earths, from 6 or 7 up to as much as 11%. They also contain subordinate proportions of lead, and the largest amount of helium. In bröggerite and cleveite, however, lead is in excess of thoria. The massive pitchblendes, on the other hand, contain no thoria, usually much lead and little or no helium. That from Black Hawk, Colorado, is exceptional. It is intimately associated with sulphide ores, but contains little lead, and zirconia instead of thoria. Carnotite, which is quite unlike uraninite, is essentially a vanadate of uranium and potassium, with very little lead and no helium. It is, however, an important source of radium.

It is now possible to correlate, at least roughly, the composition of the several minerals with the determinations of the atomic weight of uranium lead, although for a perfect comparison we should have analyses of the actual ores from which the various samples of lead were obtained. On theoretical grounds it is supposed that the true atomic weight of uranium lead is not far from 206, and only determinations which approach that value are those which represent crystallized uraninite, including the varieties bröggerite and cleveite. These minerals all contain helium, so that there seems to be a relation between the formation of these two degradation products of uranium. The

minerals also contain thorium, which would tend to raise the atomic weight and so complicate any discussion of the figures. The most brilliantly crystallized uraninite, that from Branchville, Connecticut, contains 85% of $UO_3 + UO_2$, with about 7% of ThO_2, 4.35% of PbO, and a maximum, 0.4% of helium. The atomic weight of lead from that source, unfortunately, has not been determined; and it is doubtful whether material enough for accurate investigation could be obtained.

The other determinations of the atomic weight of uranium lead give values much above 206, and even approaching 207. This is especially true of the lead from pitchblende, which contains no thorium and little if any helium. Its association with sulphide ores, however, leads to the suspicion that it may contain ordinary lead, perhaps in the form of occluded or dissolved galena. The atomic weight of the lead derived from it would, therefore, be that of a mixture, and not of the isotope alone. The carnotite lead would also seem to be a mixture, but of what kind is not clear.

The atomic weight of isotopic lead now seems to be a complex of at least three quantities, namely, the atomic weights of normal lead, uranium lead, and thorium lead, in varying proportions. Since the atomic weights of the two isotopes differ from that of normal lead in opposite directions it is difficult to determine in any particular case the relative proportions of the three modifications of the element. It has been suggested that normal lead is a balanced mixture of its isotopes; but the constancy of the atomic weight of the ordinary metal seems to negative that supposition. In order to fulfill this condition it would be necessary that the isotopes should always commingle in equal or at least definite proportions; which is extremely improbable. The apparent variations in the atomic weight of lead, as shown in the older determinations, are due to varying methods, imperfect technique, different values for the atomic weights of the other elements with which that of lead is compared, and experimental errors. The modern determinations, which I have already cited, are the only ones that are strictly comparable.

The suggestion that the lead contained in uranium ores is partly normal lead is not new. It has been advanced by other writers,[10] but the variable atomic weight of uranium lead gives the supposition a decided emphasis. It now acquires new importance because of its bearing upon certain attempts to use the ratio between uranium and lead in uranium minerals as a datum for computing the age of the earth. For this purpose the ratio has been employed by Boltwood,[11] who calculated it from almost all the trustworthy analyses of uraninite and its nearly allied species, and from it deduced their ages. These ages differ exceedingly. For a crystallized uraninite from Connecticut he found the age to be 410,000,000 years, and for Ceylonese thorianite 2,200,000,000 years. These calculations, and others, like them, involve two assumptions; first, that the rate of change from uranium to lead is accurately known, and secondly that all the lead was of radioactive origin.

The latter assumption is now seen to be extremely doubtful for the varying atomic weights prove that more than one kind of lead must be considered. Thorium lead especially must be taken into account, for many uraninites contain it, and in thorianite the percentage of thoria is more than five times that of uranium oxide. The ratio of lead to its parent elements is therefore much less than Boltwood assumed, and the calculated age of thorianite is vastly reduced. Boltwood, however, doubted the derivation of lead from thorium, a fact which was not definitely known at the time his paper was written. The evidence of the atomic weights is also much later.

Furthermore, the doubtful applicability of Boltwood's method to chronological measurements has been shown by G. F. Becker;[12] who applied it to the analyses of rare-earth minerals from one locality in Llano County, Texas. The figures given by Becker are as follows:

Mineral	*Analyst*	*Calculated age in years*
Yttrialite	Mackintosh	11,470,000,000
Yttrialite	Hillebrand	5,136,000,000
Mackintoshite	Hillebrand	3,894,000,000
Nivenite	Mackintosh	1,671,000,000
Fergusonite	Mackintosh	10,350,000,000
Fergusonite	Mackintosh	2,967,000,000

These ages differ enormously, even between two analyses of the same mineral. This evidence, taken together with the evidence from the atomic weights, seems clearly to show that the uranium-lead ratio is not applicable to the determination of the age of minerals. It is quite certain that not all of the lead in uranium ores is of radioactive origin. In pitchblende, for example, which contains no thorium, the determinations of atomic weight range from 206.40 to 206.88, figures far in excess of the theoretical 206.00 which is assigned to pure uranium lead. Normal lead, perhaps in solid solution, must be present in such ores.

What, now, is the fundamental difference between normal lead and isotopic lead? The answer to that question must be largely speculative; but speculation is legitimate when its purpose is to stimulate future research. One difference at least may reasonably be assumed, namely, that normal lead is the product of an orderly evolution of the chemical elements; and that isotopic lead is a product of their decay. Creation is one process, destruction is the other.

Forty-five years ago[13] I ventured to suggest that an evolution of the elements had actually occurred. It was clearly indicated by the progressive chemical complexity of the heavenly bodies, from the chemically simple gaseous nebulae, through the hotter stars and the sun, to the finished planets like our earth. At first, hydrogen and helium were the most abundant and conspicuous elements, then elements of higher atomic weight gradually appeared, and at the end of the process there was the chemical complexity of the earth, in which the free elements had in great part been absorbed

and replaced by a multitude of compounds. On this basis hydrogen and helium seem to be the oldest of the known elements, while uranium and thorium are the youngest of all. Lead is older than uranium and thorium, for its lines appear in the solar spectrum, in which the other two elements have not as yet been recognized. Lead, however, is vastly more abundant than either uranium or thorium, and is more likely to have been originally their progenitor than their child.

Up to this point we have a reasonable interpretation of definite evidence, beyond this, imagination must come into play. It is fair to assume that the process of evolution was extremely slow, and that each element was developed gradually and passed from an unfinished to a finished stage. The chemical atoms are now known to be extremely complex structures, each with an electropositive nucleus surrounded by electrons in rapid motion. That such a structure could have been developed instantaneously, with no previous preparation, is hardly probable, for the process was one of condensation, from lighter to heavier, and that, it would seem, must have acquired time. The process was one from relative simplicity of structure to relative complexity, and with the maximum condensation, as shown by uranium and thorium, a minimum of stability was reached. That is, so far as we now know; for less stable atoms may have been formed, to exist for a brief period and then vanish. Some of the radioactive elements which appear as products of the decay of uranium are of this kind. On that theme, more later.

That the atoms of the elements above helium in the scale of atomic weights could not have been formed instantaneously is indicated by their structure. It has been shown that they are built up of smaller particles, of electrons, and also in part, perhaps, of preexistent helium. Such particles, approaching one another, at first in irregular proportions, are supposed to have formed the atoms in question; but that exactly the right proportions for stability were found at once is hardly conceivable. There must have been a period of selection, in which the unavailable particles were discarded, probably to be used in other structures later. For each new chemical atom a definite balance between electropositive and electronegative particles was required, and also the establishment of a stable configuration. When these conditions were fulfilled the atom of an element was complete. As I have already said we can fairly assume that there was a distinct passage from an unfinished or incipient structure to a finished one of permanent stability. Furthermore, as shown by the spectra of stars and nebulae, the elements of relatively low atomic weight were first formed, and those of higher atomic weight came later. The older elements were also developed in the largest quantities, and are therefore the most abundant. The later elements are as a broad general rule much scarcer. This rule is not absolutely exact, but it expresses some well known general relations. The very simple and very stable primordial

helium, however, is now relatively rare; but there is evidence to make us believe that it was largely consumed in building other elements. Its present observed emission by radium is evidence in favor of this supposition.

In this evolutionary hypothesis with its subsidiary speculations there is, I think, nothing incompatible with present knowledge. In matters of detail it is unavoidably incomplete; but notwithstanding its imperfections it bears very directly upon a consideration of the later phenomena of radioactive decay. Here the process of evolution is reversed and rapid changes take the place of slow ones. Furthermore, the normal elements are supposed to be veritable store-houses of potential energy; which, in radioactive changes becomes partly kinetic. Radium, for example, gives forth heat continuously; and its rate of decay can be observed in the laboratory.

Through the investigation of radioactive transformations more than thirty new substances, elements or pseudo-elements, have been discovered. Some of these are extremely evanescent, lasting only for seconds or even fractions of a second; others are relatively long lived. All of them, however, are more or less unstable, and change, slowly or swiftly, into other things. Some of them are metallic, like radium, polonium, and actinium; others appear as emanations which belong to the group of the chemically inert gases. One of these, helium, is continuously being generated from radium. Some, again, are isotopic with bismuth or thallium; and four of them are said to be isotopes of lead. These are Radium B, Thorium B, Actinium B, and Radium D. The first three are short lived, and endure only for a few minutes or hours, but Radium D, also known as radio-lead, is assigned a probable life period of 24 years, and given theoretically an atomic weight not far from 210. In its chemical relations it cannot be distinguished from lead.

All four of these isotopes may have been present in uranium lead at the time of its formation, but it does not seem possible that even a trace of them could persist in the lead which is now extracted from uraninite or thorianite. They are therefore negligible in our consideration of the evidence which is now supplied by the study of the atomic weights, except in so far as they show the probable derivation of uranium lead and thorium lead from the two higher elements. The essential point is that all these varieties of lead are products of degradation, and in that respect differ fundamentally from the normal product of evolution. The thirty or more new substances which have been revealed to us by the study of radioactivity are all matter in a a state of transition from instability towards some stable form, which may be lead, or bismuth, or thallium, or some other element which has not yet been recognized as an end product of these mysterious changes. As these products are approached we have them in an incomplete condition, nearly but not quite identical with the permanent elements: This may be the character of isotopic lead. The fact that uranium lead is radioactive shows that it is still undergoing change; and that its atoms have not acquired the

exact composition and configuration which give to normal lead its uniformity and stability. Whether or not the process of change can continue until normal lead is formed, it is impossible for us to say.

The atoms of the chemical elements, are, as I have already said, extremely complex, but their structure is not yet completely understood. To some part of each kind of atom its chemical properties and its spectrum are probably due. It is conceivable that this part may be the earliest to form, with its surrounding rings or envelopes at first not quite adjusted to permanent stability. With the final adjustment the isotopes as such should disappear, and the normal element be completed. This is speculation, and its legitimacy remains to be established. A careful comparison of the spectra of the elements from thallium up to uranium might furnish some evidence as to its validity. The spectrum of uranium, for example, may contain lines which really belong to some of its derivatives.

Note.—Since this paper was written, one by Professor Barrell[14] has appeared, in which the use of the uranium-lead ratio for determining the age of minerals is defended. There are also two papers by Holmes and Lawson,[15] and another by Holmes,[16] in which the same position is taken. There is evidently room for further discussion of the subject, but as yet I see no good reason to change my own views.

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[1] *J. Amer. Chem. Soc., Easton, Pa.*, **37**, 1915, (1027).
[2] *Ibid.*, **36**, 1914, (1329).
[3] *Ibid.*, **38**, 1916, (2613).
[4] *Ibid.*, **39**, 1917, (531).
[5] *Zsch. Elektrochem., Halle*, **20**, 1914, (457).
[6] *Monatsh. Chem., Vienna*, **36**, 1915, (355).
[7] *Nature, London*, **94**, 1915, (615).
[8] *Chem. Abst.*, **11**, 1917, (3173). From *Physik. Zs.*, **18**, 1917, (114).
[9] *Washington, U. S. Geol. Survey, Bulls.* 78 and 90. Also in *Bull.* 591, pp. 366–368. Hillebrand discusses the mode of occurrence of these minerals, much as I have done.
[10] See Joly, *Phil. Mag.*, (6), **22**, 1911, (354). and Becker, *Bull. Geol. Soc. Amer.*, **19**, 1908, (134).
[11] *Amer. J. Sci., New Haven*, (4), **23**, 1907, (86).
[12] *Bull. Geol. Soc. Amer.*, (4), **19**, 1908, (134). See also Zambonini, *Rome, Atti Acc. Lincei* (5), **20**, part 2, 1911, (131).
[13] *Popular Science Monthly*, January, 1873.
[14] *Bull. Geol. Soc. Amer.*, **28**, 1918, (745).
[15] *Phil. Mag.*, (6), **28**, 1914, (823), **29**, 1915, (682).
[16] *Proc. Geologist's Assoc., London*, **26**, 1915, (289).